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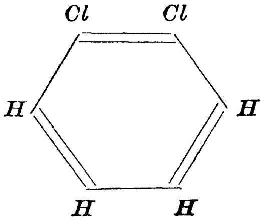
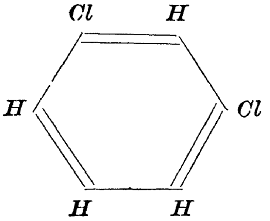
EXTRACT FROM A LETTER OF DR. FRANKLAND TO MR. SYLVESTER.

. . . I WANT now to offer one or two critical remarks upon your papers. . . . At page 65 you notice the anomaly of a *free* bond. My system of notation repudiates all free bonds; but I fear the expression may sometimes have escaped me in describing the transference of an element or compound radical from one compound to another; but I only meant the breaking of the bond for an infinitely short time. I think you also employ the corresponding term "free valence" in this sense at page 69. For free valence, however, I should substitute "latent valence." With very few exceptions, this latent valence is always an even number, hence my hypothesis of two bonds saturating each other (see page 21 of my Lecture Notes).

At page 71, I see you prefer Ladenburg's prism or hexagon to Kekulé's graph for benzol.[*] It certainly accords better with facts. The facts are 1st. When 1, 5 or 6 atoms of hydrogen in benzol are replaced by *Cl*, *Br*, &c., there are no isomers. 2d. When 2, 3 or 4 atoms are so replaced, there are always 3 isomers and three only. Now, Kekulé's graph exhibits the possibility of four different positions for 2, 3 or 4 atoms of *Cl*, and hence of 4 isomers.



No. 1.



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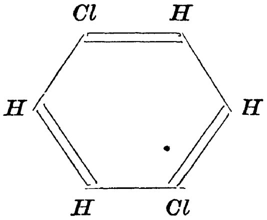
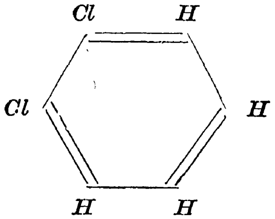


No. 3.



No. 4.

Kekulé does not seem to have noticed the difference between No. 1 and No. 4.

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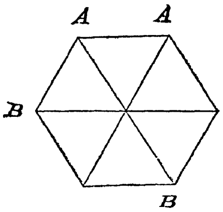
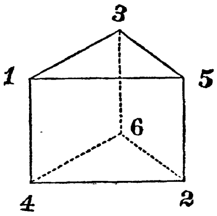
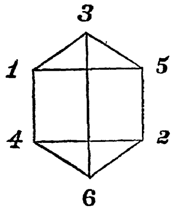
[*] I did not intend to express any such preference, but merely to state the mathematical fact that the graph of the one is a graph of a covariant belonging to a single biquadratic form which the other is not. The truth is, that chemical graphs correspond to the case of an indefinite repetition of each algebraical form as would necessarily be the case if each chemical atom were capable of playing a different part, or, at all events, of being distinguished in character from every other of the same name with which it may be associated. Mr. Lockyer's recent discoveries seem to favor the notion of this being the case for hydrogen. J. J. S.



No. 5.

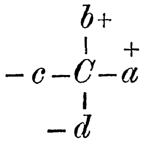


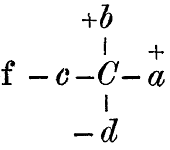
No. 6.

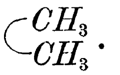
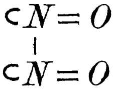


No. 7.

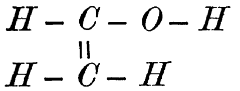
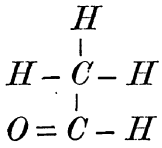
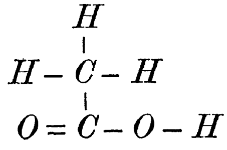
On the other hand, the graph, No. 5, will not answer, because it only allows of two different positions, *AA* and *BB*, unless indeed you assign a different value to the cross bonds from that of the lateral ones; but No 6 gives three different positions, and three only, viz: 1st. Either 1–2, 2–3, 3–4, 4–5, 5–6, or 6–1, for all these pairs are obviously identical. 2d. Either 1–3, 3–5, 5–1, 2–4, 4–6, or 6–2, for these positions again are identical; and 3d. Either 1–4, 2–5, or 3–6, which are also identical. We are now accustomed to call the first set of positions "Ortho," the second "Meta" and the third "Para." I quite agree with you that all graphs may be represented by figures in a plane. Thus, No. 6 may be drawn as No. 7. It is very amusing to see the figure worship of some chemists, and the horror of it in others. Both seem to be unaware that graphic formulæ are only symbols of phenomena, and have no connection whatever with space........

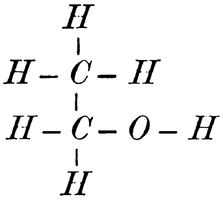
I was at first inclined to smile at your invention of a new set of atoms (atomicules); but, on further consideration, it seems to me that your conception of the constitution of atoms may prove of great value, not in its application to variation of atomicity, but in furnishing an explanation of the behaviour of certain atoms which has long puzzled me and doubtless also other chemists. Carbon affords the best example of what I mean. The strong affinity of carbon for carbon is a quality upon which depends the very existence of nearly every organic compound, and sharply distinguishes these compounds from the almost equally complex siliceous minerals, in which the atoms of silicon are, in no single instance, combined with each other. The most obvious explanation would be that the bonds of carbon are + and — to each other, whilst those of silicon are identical or endowed with energy of the same kind and intensity. But it is a remarkable fact that hitherto, with very few and dubious exceptions, all attempts to establish a difference between the 4 bonds of the carbon atom have failed. Thus, if an atom of carbon be combined with 4 atoms or monad groups *a*, *b*, *c*, *d*,  $-c-\overset{\overset{b+}{|}}{\underset{\underset{-d}{|}}{C}}\overset{+}{-}a$ of which *a* and *b*

are + and c and d —, it does not matter apparently in what order these bodies are introduced to the carbon atom; for instance, if the body $\overset{+}{a}$ be replaced by a negative body $\overset{-}{e}$, and $\overset{-}{c}$ by a + body $\overset{+}{f}$; and if then in another molecule of 
$$\begin{array}{c} +b \\ | \\ -c-C-\overset{+}{a} \\ | \\ -d \end{array}$$
, $\overset{-}{c}$ be replaced by $\overset{-}{e}$, and $\overset{+}{a}$ by $\overset{+}{f}$, the two new molecules are identical and not isomeric as might be expected. But if, according to your conception, each bond is tripartite, it could of course exert either + or — energy according to the quality of the atom or group presented to it.

The application of the theory of atomicules to explain latent bonds is of course perfectly legitimate, but I cannot see its advantage over the hypothesis of mutual neutralization. It has never occurred to me that there is any more difficulty in thinking of carbonic oxide as $CC=O$ than in conceiving methyl as 
$$\left\langle\begin{array}{l} CH_3 \\ CH_3 \end{array}\right.$$
. If two bonds of two separate but perfectly similar atoms of carbon can satisfy each other, why not two bonds in one and the same atom? If you could apply your theory of atomicules to explain the small number of anomalies which have hitherto defied all other theories, chemistry would be greatly indebted to you. Thus, nitric oxide, if written $N=O$, as required by Avogadro's law, violates the law of variation of atomicity, for a perissad element becomes an artiad. If, on the other hand, it be written, as in my notes, 
$$\begin{array}{l} \subset N=O \\ \;| \\ \subset N=O \end{array}$$
it violates Avogadro's law for equal volumes of hydrogen and nitric oxide do not contain an equal number of molecules.

In reference to your remark on p. 83, every chemico-graph is assumed to be possible, but, in many cases, it is known to be incapable of existing under the conditions prevailing at the surface of the earth.

At page 108, you seem to arrive, by a mathematical process, at 
$$\begin{array}{l} H-C-O-H \\ \quad \| \\ H-C-H \end{array}$$
as the graphic formula of aldehyde; but this is the formula assigned by chemists to vinylic alcohol, which appears, however, to be incapable of existence. The formula universally recognized for aldehyde is 
$$\begin{array}{c} H \\ | \\ H-C-H \\ | \\ O=C-H \end{array}$$
which makes its relations to acetic acid 
$$\begin{array}{c} H \\ | \\ H-C-H \\ | \\ O=C-O-H \end{array}$$
and

alcohol 

$$\begin{array}{ccccccc} & & H & & & & \\ & & | & & & & \\ H & - & C & - & H & & \\ & & | & & & & \\ H & - & C & - & O & - & H \\ & & | & & & & \\ & & H & & & & \end{array}$$

simpler than yours.[*]

The law you mention at page 114, "*m n–valent atoms may be replaced by n m–valent ones*," has a very wide application in chemistry.

I do not think that we have any trustworthy evidence of the difference of H atoms mentioned at page 123.

I cannot, like Mr. Mallet, conceive of "centres of force," or indeed of force at all, apart from matter.

In reference to the origin of the theory of atomicity, I send you, by book post, a copy of a paper of mine presented to the Royal Society, on May 10th 1852, in which, at page 438, *et seq.*, you will find what, so far as I know, was the first enunciation of the theory of atomicity. After some preliminary explanations the law is expressed as follows, at page 440: "Without offering any hypothesis regarding the cause of this symmetrical grouping of atoms, it is sufficiently evident, from the examples just given, that such a tendency or law prevails, and that, *no matter what the character of the uniting atoms may be, the combining power of the attracting element, if I may be allowed the term, is always satisfied by the same number of these atoms.*" I then go on to apply the theory to the explanation of the constitution of organo-metallic compounds. It was in fact the discovery of these bodies which first forced the law upon my attention; for I was surprised to find that the more methyl or ethyl I combined with a metal, the less room was there for O, Cl, &c., although the energy with which the ethylised metal combined with these elements was enormously greater than before ethylisation, some of the ethylised metals being even spontaneously inflammable. If you remember that what I call "the uniting atoms" were monads, and that nearly all chemists then regarded the atom of oxygen (with the atomic weight of 8) as equivalent to one atom of hydrogen, you will readily understand the examples I give. The application of the theory to carbon compounds generally, was made by Kolbe and myself in December, 1856 (see Ann. der Chemie. und Pharm., Bd. CI, s. 257), the first word from Kekulé on the subject of atomicity appearing about a year later. I have ordered my publisher to send a copy of my "Experimental Researches" to the Library of the Johns Hopkins University. If you will kindly refer to pages 145 and 154, you will see what I have said on the subject last year, whilst at page 188 you will find the paragraphs in my Royal Society memoir, and at page 148 the manifesto of Kolbe and myself which discloses the application of the doctrine of atomicity, or atom fixing power, to the compounds of *C*. Look especially at page 150 for the declaration of Kolbe's conversion to the theory. All this happened, as I have said, long before Kekulé took up the matter; but his subsequent work, and especially the later rectification of atomic weights by Cannizzaro, aided the further development of the theory enormously; but it is singular that no subsequent writer on the subject appears to have known of my paper in the Philosophical Transactions.

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[*] If my memory is not in fault, the form I have given is the only graph possible if each carbon atom refers to the same biquadratic, but there seems no reason to suppose that such is the case in nature. (*Vide* foot-note, page 345.) J. J. S.

I trust that you will go on with the consideration of chemical phenomena from a mathematical point of view, for I am convinced that the future progress of chemistry, as an exact science, depends very much indeed upon the alliance of mathematics. . . . . . .

LONDON, *October 13, 1878.*